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THE OUTLOOK FOR APPLIED ENTOMOLOGY.[1]

International Interests.

WITH the constantly increasing facilities for intercommunication between different parts of the globe, the results obtained and experiences had in one part are soon available for the rest of the world. Thus France has more than repaid the United States for the good, however vast and important, that has resulted to her by the use of American resistant stocks. Her experience with these American vines has reacted beneficially upon our own viticulture in many directions, but particularly in the great advance which her sons have made in insecticides and fungicides, and in convenient, portable insecticide and fungicide appliances. It has often been said of the French that they are not an originating people. However that may be, they are very quick at adopting and improving ideas and discoveries once brought to their notice, and no nation is more appreciative of the immense practical benefits to be received by the adoption of the most scientific methods. In fact, no nation has given greater government incentive to the pursuit of science in its bearings upon the welfare of mankind, and we may study with profit what she has of late years done in our own line.

I had a delightful visit last August from Mr. John West, who came to this country as a delegate from Victoria to ascertain all he could of our methods; also from Mr. W. Catton Gasby of Adelaide, who also visited this country in a similar capacity. Economic entomology in their part of the world is extremely interesting to us; for while the seasons are reversed, as compared with ours, many of the same injurious insects occur in both countries. Thus I was glad to get perfect confirmation from Mr. West of the fact that the Northern Spy and the Winter Majetin are found to protect the apples grafted upon them from the woolly *Aphis*. A great deal has been published of late years in the New Zealand and Australian papers on "blight-proof" apple stock, and they have had an important experienee, the outcome of sore necessity, for *Schizoneura lanigera* has there been one of the most serious drawbacks to apple-culture.

There can be no question but that this experience will prove of value to our apple growers wherever these varieties succeed and the woolly *Aphis* abounds. The use, as stocks, of such varieties as enjoy immunity from the woolly *Aphis*, has occurred to our own people, but no such extended experience has been had in regard to any particular resistant variety. Some of our injurious insects are often worse in Australia than they are with us, and we may expect to reap the benefit of the experience had there with regard to them. This will doubtless be true not only of the codling-moth, but of their peach *Aphis*, which, from all that I can learn, is evidently the same species as that which does so much damage in our lighter soils along the Atlantic coast, and which Dr. Erwin F. Smith of the Division of Mycology of the department at Washington has studied lately, and described in great detail as a new species under the name of *Aphis persicæ-niger*, but which I have reason to believe is the *Aphis prunicola* of Kaltenbach.

The Italians have been making a very interesting fight against an insect which has threatened their very important and extensive silk industry by its attacks upon the mulberry-tree. This insect was described by Targoni Tozzetti in 1885 as *Diaspis pentagona*. It occurs upon a number of different trees, among them the paper mulberry, the spindle-tree, the peach, the cherry, laurel, and certain willows, as well as upon the cultivated white mulberry; and it would seem that its taste for the last-named tree is one recently acquired, judging from the late date at which the habit has attracted attention. The energetic director of the Entomological Experiment Station at Florence investigated the species in 1886, and recommended the use of mechanical means at the time of hatching of the young; viz., the scrubbing of the trunks and larger branches with stiff brushes, and a subsequent application of a mixture of soap and water with four or five per cent of kerosene.

Professor Franceschini, the editor of the *Rivista de Bachicoltura*, recommended the adoption of the Balbiani formula as used against *Phylloxera*, consisting of crude tar-oil, naphthaline, quick-lime, and water; the naphthaline being dissolved in the tar-oil, and the water and lime afterward added together. The insect appeared first in several cantons of the province of Como, and speedily spread to the adjoining localities. The matter was brought to the attention of the Ministry of Agriculture, and a commission was appointed consisting of Professor Targoni Tozzetti, Dr. Alpe, and Dr. Andres, who immediately familiarized themselves with the methods in use in this country, and have made extensive experiments with our kerosene emulsion, with our fumigating processes, and with other new remedies. The subject has been taken in hand with great vigor, and the government has interested itself to the extent of appointing inspectors in the different communes in the infested territory, and establishing regulations which oblige the immediate report of new localities and the adoption of measures of extinction when ordered by inspectors. These regulations also provide that the inspectors must do the work at the expense of proprietors when the latter refuse to do so. They prohibit the exportation of leaves from infested localities to others, and provide for indemnity to owners for the destruction of trees when the degree of infection is such as not to threaten the ultimate life of the trees. Expenses for experiments of all kinds, and for the watching and care exercised by agents, are borne by

[1] Continued from p. 20, Science, Jan. 9.

the State; while the expense for the execution of certain of the regulations is borne, one third by the proprietor, and two thirds by the local society. A fine for disobedience of the regulations is also provided for. The laws, as published, are none too severe, and meet the urgency of the case; and it is refreshing to notice the energy with which the government has met the threatened danger, and at the same time gratifying to note the appreciation shown of our own means and methods.

Use of Contagious Germs in the Field.

Most of you are aware that I have not had the greatest faith in the availability of contagious disease-germs as a means of battling with injurious insects in field, garden, orchard, or forest. There are so many delicate questions involved, and so many obstacles in the way of practically carrying out any plan, however plausible theoretically, or true in principle! Our ability to contaminate healthy by diseased specimens is but a short step, and leaves many important questions, as of rapid dissemination, untouched. The theory is very tempting, and has been particularly dwelt upon by some who were essentially closet-workers, having but faint realization of the practical necessities of the case. Theoretically, with those insect diseases of a cryptogamic nature, having a complex life-history and a resting spore, the difficulties are greater than with those of a bacterial origin; and it is to these last that we should look for important aid, if it be available. Yet if the work of Messrs. Lugger and Snow should be fully substantiated, the best results have so far been obtained with the entomophthora of the chinch-bug. No one will be more pleased to have his doubts dissipated by some tangible evidence of the practicability of this method than myself. Success, if possible, will come only by investigations upon thoroughly careful and scientific lines, such as those begun and still pursued by Professor Forbes. The ease with which he conveyed the silk-worm *pébrine* to other larvæ, his conveying the cabbage-worm micrococcus to other larvæ, and his carrying this micrococcus in cultures over winter, are promising facts, as is also Professor Osborn's contaminating cabbage-worms in Iowa with specimens brought from Illinois. Congress having at its last session appropriated twenty-five hundred dollars for some further investigation of the boll-worm, the possibilities in this direction for this particular insect have caused me to plan investigations having for their object thorough field experiment with some of these disease-germs.

Heliothis armigera is one of those cosmopolitan insects which has become more injurious in the United States than in any other part of the world, by virtue of its partiality for green corn, green cotton-bolls, and green tomatoes. The polyphagous and partially endophytous habit of the larva renders its destruction difficult, except during the earlier free-living stages, by the fine spraying of the arsenites on the under surfaces of the leaves. The ideal treatment for the larger burrowing worms were some rapidly spreading disease-germ that would penetrate and destroy them in their hidden recesses. The insect was reported as extremely abundant in cotton-bolls during the summer, especially in Texas; but by the time the appropriations became available, its numbers had decreased, and it was too late in the season to do much more than prepare for next year. We may expect, as a result of special investigation, much additional fact and experience as to habits, natural enemies, and means of controlling; but it is my desire to make the trial of these disease-germs the special feature of the investigation. Of those employed in the investigation, Mr. F. W. Mally was a former assistant to Professor Forbes, and has some experience in the study and culture of disease-germs; while Dr. A. R. Booth is something of an enthusiast on the subject, and has already established the susceptibility, through contact, of the boll-worm to the cabbage-worm micrococcus (*M. pieridis*) of Burrill, and is preparing to carry the germs through the winter. I have had in mind, as probably the most promising germ, that which affects *Nephelodes violans* in a similar epidemic way, but which, as Professor Forbes informs me, is a quite distinct micrococcus, and shall be pleased to have any of you co-operate with me next year by informing me of any disease of this character that may prevail in your several localities.

Apiculture.

While little attention has so far been given by the different stations to the subject of apiculture, except at Lansing, it is nevertheless an important branch of economic entomology, and there is much promise of good results yet to come from careful experiment and investigation. One of the most inviting fields is the search for and introduction of new varieties or species of bees; for just as American apiculture has profited in the past by the importation of races like the Italians, Syrians, and Carniolans, there is every prospect of further improvement by the study and introduction of such promising races as are either known to occur or may be found, in parts of Africa and Asia. *Apis dorsata* is believed to have many desirable qualities; and private efforts have already been made to introduce it, and have failed chiefly for want of means. The further study of desirable bee forage-plants, and the introduction and acclimatization of such as are known to be valuable in parts of the country where they do not yet occur, are very desirable.

Much has yet to be done, also, in the line of systematic breeding; and we should be able to make rapid advances in the amelioration of existing races by proper selection, if we could assume practical and ready control of the fertilization of the queen. In these directions we are now planning at the department some effective work; but the introduction of foreign bees, which the department should be able to undertake to better advantage than any private individual or State institution, is rendered more difficult by virtue of the restrictions in the appropriation already alluded to in discussing the subject of the introduction of parasites; and whatever is done in the other directions by the national department will be done most advantageously through the co-operation of one or more of the State stations, many of which are far better equipped and more favorably situated for apicultural work than the department at Washington.

Silk-Culture.

This, again, is an important part of applied entomology, and, as most of you know, I have for many years worked toward the establishment of silk-culture in this country. The result of these efforts has served only to convince me of the utter impossibility of successfully entering upon the enterprise on a business basis, without protective duty on the reeled or misnamed "raw" silk. Some five years ago, largely through the then commissioner's appeal, based on my own report and assurances, Congress appropriated fifteen thousand dollars for the express purpose of giving a thorough test to the Serrell automatic reeling-machinery, in the hope that by its means the question of labor might be minimized, and we could reel silk at a profit. The previous attempts of

the department, which it had been my lot to direct, of establishing such reeling or market centres at San Francisco, New Orleans, and Philadelphia, had proved unsuccessful; and the promise was made to Congress that two years of experimentation under my immediate direction at Washington would enable a definite decision of the question. Two years passed, and the appropriation was increased, and continued a third year, for various reasons stated at the time. At the end of the third year I became convinced of the futility of continuing the experiments indicated without protective duty, and so stated in my report. While in Europe, in 1889, I paid particular attention to the question, and visited the Serrell works at the Serrell establishment at Chabeuil, where I found that Mr. Serrell had abandoned his own reeling-machinery, which was stored in the cellar, and had gone back to the use of the ordinary non-automatic reeling-machines, though employing improved automatic brushes and cleaners of his own invention, which have such advantages that they are fast coming into use in France and Italy. I felt more convinced than ever of the futility of continuing the experiments at Washington, except with the protection indicated, especially as any improvement or valuable outcome of such experiments would redound primarily to the benefit of a private corporation, and doubtless benefit other countries more than our own. The hope of improvement, and the attractiveness of the machinery to the average visitor, among other reasons, to which I need not now refer, have caused continuation of the special reeling-work against my advice. From the foregoing you will naturally draw the conclusion that I do not at present favor any time being wasted on the subject at the State stations, since Congress declined to put a duty on "raw" silk, — a striking illustration of the inconsistencies of the tariff schedule.

Legislation.

The amount of legislation in different countries that has of late years been deemed necessary or sufficiently important, in view of injurious insects, is a striking evidence of the increased attention paid to applied entomology; and while modern legislation of this kind has been, on the whole, far more intelligent than similar efforts in years gone by, many of the laws passed have nevertheless been unwise, futile, and impracticable, and even unnecessarily oppressive to other interests. The chief danger here is the intervention of politics or political methods. Expert council should guide our legislators, and the steps taken should be thorough in order to be effective. We have had of late years in Germany very good evidence of the excellent results flowing from thorough methods; and the recent legislation in Massachusetts against the gypsy-moth (*Ocneria dispar*), which at one time threatened to become farcical, has fortunately proved more than usually successful, the commission appointed to deal with the subject having worked with energy, and followed competent advice.

Publication.

On the question of publication of the results of our labors, it is perhaps premature to dwell at length. Each of the experiment stations is publishing its own bulletins and reports quite independently of the others; but after a uniform plan recommended by the association with which we meet here, and with few exceptions that have come to my notice, another important recommendation of the same association — that these publications shall be void of all personal matter — has been kept in mind. The National Bureau of Experiment Stations at Washington is doing what it can with the means at command to further the general work by issuing the experiment-station record, devoted chiefly to digests of the State station bulletins. There is a serious question in my mind as to the utility of State digests by the national department, of results already published extensively by the different States, and distributed under government frank to all similar institutions and to whomsoever is interested enough to ask for them. Such digests may or may not be intelligently made, and, even under the most favorable circumstances, will hardly serve any other purpose than helping to the reference to the original articles; and this could undoubtedly be done more satisfactorily to the stations, and to the people at large, by general and classified indexes to all the State documents, made as full as possible, and issued at stated intervals. Only a small proportion of the bulletins have been so far noticed by digest in this record, with no particular rule, so far as I can see, in the selection. This is, perhaps, inevitable under present arrangements. Complete and satisfactory digests of all, if intelligent and critical, imply a far greater force than is at present at Professor Atwater's command, and it is doubtful whether, even with increased facilities, they could be satisfactorily made without the assistance of the different specialists.

Under these circumstances, it would seem wiser to devote all the energies of the bureau to digests of the similar literature of other countries, which would be of immense advantage to our people and to the different station workers. Judging from the recommendations and resolutions of the general association, this is the view very generally held; but except in chemistry, and special industries like that of beet-sugar, very little of that kind of work has yet been attempted.

What is true of the station publications in general is equally true of special publications. As entomologist of the department, I have been urged to bring together at stated intervals digests of the entomological publications of the different stations. Such digests, to be of any value, however, should also be critical; but it is, at best, a thankless task for any one to be critic or censor even of that which needs correction or criticism, and also difficult to maintain the judicial and impersonal attitude which should characterize official expression, in face of the severe criticism that some publications provoke. Moreover, to do this work intelligently would require increase of the divisional force, which at present is more advantageously employed, for, as already intimated, I should have great doubts of the utility of these digests.

I believe, however, that the division should strive for such increase of means as would justify the periodic publication, either independently or as a part of the department record, of general and classified indexes to the entomological matter of the station bulletins, and should work more and more toward giving results from other parts of the world. This could perhaps best be done by titles of subject and of author, so spaced (and printed on stout paper) that they could be cut and used in the ordinary card catalogue. The recipient could cut and systematically place the titles as fast as received.

As to the character of the matter of the entomological bulletins, it will inevitably be influenced by the needs and demands of the people of the respective States, and, while originality should be kept in mind, there must needs be in the earlier years of the work much re-statement of what is already well known. That some results have been published of work which reflects no particular credit upon our

calling, is a mere incident of the new positions created; yet we may expect marked improvement from year to year in this direction. Without being invidious, I would cite those of Professor Gillette, on his spraying experiments and on the plum curculio and plum gouger, as models of what such bulletins should be.

Although the resolution offered at our last meeting by Professor Cook, to the effect that purely descriptive matter should be excluded from the station bulletins, met with no favor, but was laid on the table by the general association, I am in full sympathy with this position, and am strongly of the opinion that in the ordinary bulletins such purely technical and descriptive matter should be reduced to the necessary minimum consistent with clearness of statement and accuracy, and that if it is desired, on the part of the station entomologists, to issue technical and descriptive papers, a separate series of bulletins were better instituted for this class of matter.

Finally, for results which it is desired to get promptly before the people, the agricultural press is at our disposal; and, so far as the entomological work of the Department of Agriculture is concerned, the periodical bulletin, *Insect Life*, was established for this purpose. Its columns are open to all station workers; and I would here appeal to the members of the association to help make it, as far as possible, national, by sending brief notes and digests of their work as it progresses. Hitherto we have been unable to make as much effort in this direction as we desired; but in future it is our hope to make the bulletin, as far as possible, a national medium, through which the results of work done in all parts of the country may quickly be put on record, and distributed not only to all parts of our own country, but to all parts of the world.

The rapid growth and development of the national department, and the multiplication of its divisions, have necessitated special modes of publication, and rendered the annual report almost an anachronism, so far as its pretends to be what it at one time was, a pretty complete report of the scientific and other work of the department. The attempts which I have made through the proper authorities to get Congress to order more pretentious monographic works in quarto volume similar to those issued by other departments of the government have not met with encouragement, and in this direction many of the stations will, let us hope, be able to do better.

Co-operation.

Every other subject that might be considered on this occasion must be subordinate to the one great question of co-operation. With the large increase of actual workers in our favorite field, distributed all over the country, the necessity for some co-operation and co-ordination must be apparent to every one. Just how this should be brought about, or in what direction we may work toward it, will be for this association, in its deliberations, to decide. Nor will I venture to anticipate the deliberations and conclusions of the special committee appointed to take the matter into consideration, beyond the statement that there are many directions in which we can adopt plans for mutual benefit. Take, for instance, the introduction and dissemination of parasites. How much greater will be the chance of success in any particular case if we have all the different station entomologists interested in some specific plan to be carried out in co-operation with the national department, which ought to have better facilities of introducing specimens to foreign countries or to different sections of our own country than any of the State stations! Let us suppose that the fruit-growers of one section of the country, comprising several States in area, need the benefit in their warfare against any particularly injurious insect of such natural enemy or enemies as are known to help the fruit-growers of some other section. There will certainly be much greater chances of success in the carrying-out of any scheme of introduction, if all the workers in the one section may be called upon, through some central or national body, to help in the introduction and disposition of the desired material into the other section. Or take the case of the boll-worm investigation already alluded to. The chances of success would be much greater if the entomologists in all the States interested were to give some attention to such lepidopterous larvæ as are found to be affected with contagious diseases, and to follow out some specific plan of cultivating and transmitting them to the party or parties with whom the actual trials are intrusted. The argument applies with still greater force to any international efforts. I need hardly multiply instances. There is, it is true, nothing to prevent any individual station entomologist from requesting co-operation of the other stations, nor is there anything to prevent the national department from doing likewise; but in all organization results are more apt to flow from the power to direct rather than from mere liberty to request or to plead. The station entomologist may be engrossed in some line of research which he deems of more importance to the people of his State, and may resent being called upon to divert his energies; and, with no central or national power to decide upon plans of co-operation for the common weal, we are left to voluntary methods, mutually devised; and it is here that this association can, it seems to me, most fully justify its organization. And this brings me to the question of the department and the stations.

The Department of Agriculture and the State Stations.

Immediately connected with the question of co-operation is the relation of the National Department of Agriculture and the State experiment stations. The relation, instead of being vital and authoritative, is in reality a subordinate one. Many persons interested in the advancement of agriculture foresaw the advantage of having experiment stations attached to the State agricultural colleges founded under the Morrill Act of 1862; but I think that in the minds of most persons the establishment of these stations implied some such connection with the national department as that outlined in an address on agricultural advancement in the United States, which I had the honor to deliver in 1879 before the National Agricultural Congress at Rochester, and in which the following language was used: —

"In the light of the past history of the German experimental stations and their work, or of that in our own State of Connecticut, the expediency of purchasing an experimental farm of large dimensions in the vicinity of Washington is very questionable. There can be do doubt, however, of the value of a good experimental station there, that shall have its branches in every State of the Union. The results to flow from such stations will not depend upon the number of acres at command, and it will be far wiser and more economical for the commissioner to make each agricultural college that accepted the government endowment auxiliary to the national bureau; so that the experimental farm that is now, or should be, connected with each of these institutions, might be at its service, and under the general management of the superintendent of the main station. There is reason to believe that the directors of these colleges would cheerfully have them constituted as experimental stations under the

direction of the department, and thus help to make it really national,—the head of a vast system that should ramify through all parts of the land.

. . . . . .

"With the different State agricultural colleges, and the State agricultural societies or boards, we have every advantage for building up a national bureau of agriculture worthy of the country and its vast productive interests, and on a thoroughly economical basis, such as that of Prussia, for instance."

In short, the view in mind was something in the nature of that which has since been adopted by our neighbors of the north, where there is a central or national station or farm at Ottawa, and sub-stations or branch farms at Nappan (Nova Scotia) Brandon (Manitoba), Indian Head (N.W.T.), and Agassiz (British Columbia), all under the able direction of Mr. William Saunders, one of our esteemed fellow-workers. It was my privilege to be a good deal with Mr. Saunders when he was in Europe studying the experience of other countries in this matter; and the policy finally adopted in Canada as a result of his labors is an eminently wise one, presenting none of the difficulties and dangers which beset our plan, whether as between state and nation or college and station.

Under the present laws, and with the vast influence which the Association of Agricultural Colleges and Experiment Stations will wield both in Congress and in the different States, there is great danger of transposition, in this agricultural body politic, of those parts which in the animal body are denominated "head" and "tail;" and the old saw to the effect that "the dog wags the tail because the tail cannot wag the dog" will find another application. So far as the law goes, the national department, which should hold a truly national position towards State agricultural institutions depending on federal support, can do little except by suggestion, whether in the line of directing plans or in any way co-ordinating or controlling the work of the different stations throughout the country. The men who influenced and shaped the legislation which resulted in the Hatch Bill were careful that the department's function should be to indicate, not to dictate; to advise and assist, not to govern or regulate. We have therefore to depend on such relationships and such plans of co-operation as will appear advantageous to all concerned, and these can best be brought about through such associations as are now in convention here. Without such plans, there is great danger of such waste of energy and means and duplication of results as will bring the work into popular disfavor and invite disintegration, for already there is a growing feeling that agricultural experiment is and will be subordinated to the ordinary college-work in the disposition of the federal appropriations.

What is true of the national department as a whole in its connection with the State stations is true in a greater or less degree of the different divisions of the department in connection with the different specialists of the stations. With the multiplicity of workers in any given direction in the different States, the necessity for national work lessens. A favorite scheme of mine in the past, for instance (and one, I am glad to say, fully indorsed by Professor Willits), was to endeavor to have a permanent agent located in every section of the country that was sufficiently distinctive in its agricultural resources and climate, or, as a yet further elaboration of the same plan, one in each of the more important agricultural States. The necessity for such State agents has been lessened, if not obviated, by the Hatch Bill, and the subsequent modifications looking to permanent appropriations to the State stations or colleges, which give no central power at Washington. The question then arises, what function shall the national department perform? Its influence and field for usefulness have been lessened rather than augmented in the lines of actual investigation in very many directions. Many a State is already far better equipped as to valuable surrounding land, laboratory and library facilities, more liberal salaries and greater freedom from red tape, administrative routine, and restrictions as to expenditures, than we are at Washington; and, except as a directing agent and a useful servant, I cannot see where the future growth of the department's influence is to be outside of those federal functions which are executive. Just what that directing influence is to be is the question of the hour, not only in the broader but in the special sense. The same question in a narrower sense had arisen in the case of the few States which employed State entomologists. In the event, for instance, of an outbreak of some injurious insect, or in the event of any particular economic entomological question within the limits of the State having such an officer, the United States entomologist would naturally feel that any effort on his part would be unnecessary, or might even be looked upon as an interference. He would feel that there was always danger of mere duplication of observation or experiment, except where appealed to for aid or co-operation. This is perhaps true only of insects which are local or sectional, and is rather a narrow view of the matter; but it is one brought home from experience, and is certainly to be considered in our future plans. The favor with which the museum work of the national division was viewed by you at the meeting last November, and the amount of material sent on for determination, would indicate that the building-up of a grand national reference collection will be most useful to the station workers. But to do this satisfactorily we need your co-operation; and I appeal to all entomologists to aid in this effort by sending duplicates of their types to Washington, and thus more fully insuring against ultimate loss thereof.

Status of our Society.

This train of thought brings up the question of the status of our society with the station entomologists as represented by the committee of the general association. Those of us who had desired a national association for the various purposes for which such associations are formed, felt, I believe, if I may speak for them, that the creation of the different experiment stations rendered such an organization feasible. Your organization at Toronto, and the constitution adopted and amended at the meeting at Washington, all indicate that the chief object was the advancement of our chosen work, and that the strength of the association would come from the experiment-station entomologists. There was then no other organization of the kind, nor any intimation that such a one would be founded. Some of us, therefore, were surprised to learn from the circular sent out by Professor Forbes, its chairman, that the committee appointed by the Association of Agricultural Colleges and Experiment Stations, and through which we had hoped to communicate and co-operate with that association, was not in the proper sense a committee, but a section which has prepared (and, in fact, was required by the executive committee and the rules of the superior body to prepare) a programme of papers and discussions for the meeting, to be held at the same time and place with our own. I cannot but feel that this is, in some respects, a misfortune, and it will devolve upon you to decide upon several questions of importance that will materially

affect our future existence. There is not room for two national organizations having the same objects in view, and meeting at the same time and place, goes, I think, without saying; and if the committee of the general association is to be any thing more than a committee in the proper sense of the word, or if it is to assume with or without formal constitution the functions of our own association, then our own must necessarily be crippled, and, to do any good at all, must meet at a different time and a different place. A committee or section, or whatever it may be called, of the general association with which we meet, would preclude active membership of any but those who come within the constitution of that body. Our Canadian friends and many others who have identified themselves with applied entomology, and do not belong to any of our State or government institutions, would be debarred from active representation, however liberal the association may have been in inviting such to participate, without power to vote, in its deliberations. Our own association has, or should have, no such limitations. Some of us who are entitled to membership in both bodies may feel indifferent as to the course finally decided upon, and that it will not make any difference whether we have an outside and independent organization, as that of the Association of Official Chemists, or whether we do, as did the botanists and horticulturists, waive independence in favor of more direct connection with the general association, providing there is some way whereby the committees of the general association are given sufficient latitude and time to properly present their papers and deliberate; but there are others who feel more sensitive as to their action, and are more immediately influenced by the feelings of the main body. I hope, that, whatever action be taken at this meeting, the general good and the promotion of economic entomology will be kept in mind, and that no sectional or personal feeling will be allowed to influence our deliberations.

Suggestion and Comment.

You will, I know, pardon me if, before concluding these remarks, I venture to make a few comments which, though not altogether agreeable, are made in all sincerity, and in the hope of doing good. The question as to how far purely technical and especially descriptive and monographic work should be done by the different stations or by the national department is one which I have already alluded to, and upon which we shall probably hold differing opinions, and which will be settled according to the views of the authorities at the different stations. Individually I have ever felt that one ostensibly engaged in applied entomology, and paid by the State or National government to the end that he may benefit the agricultural community, can be true to his trust only by largely overcoming the pleasure of purely entomological work having no practical bearing. I would therefore draw the line at descriptive work, except where it is incidental to the economic work and for the purpose of giving accuracy to the popular and economic statements. This would make our work essentially biological; for all biologic investigation would be justified, not only because the life-habits of any insect, once ascertained, throw light on those of species which are closely related to it, but because we can never know when a species, at present harmless, may subsequently prove harmful, and have to be classed among the species injurious to agriculture.

On the question of credit to their original sources of results already on record, it is hardly necessary for me to advise, because good sense and the consensus of opinion will in the end justify or condemn a writer, according as he prove just and conscientious in this regard.

There is one principle that should guide every careful writer ; viz., that in any publications whatever, where facts or opinions are put forth, it should always be made clear as to which are based upon the author's personal experience, and which are compiled or stated upon the authority of others. We should have no patience with a very common tendency to set forth facts, even those relating to the most common and best-known species, without the indications to which I have referred. The tendency belittles our calling, and is generally misleading and confusing, especially for bibliographic work, and cannot be too strongly deprecated.

On this point there will hardly be any difference of opinion; but I will allude to another question of credit upon which there prevails a good deal of loose opinion and custom. It is the habit of using illustrations of other authors without any indication of their original source. This is an equally vicious custom, and one to be condemned, though I know that some have fallen into the habit without appreciation of its evil effect. It is, in my judgment, almost as blameworthy as to use the language or the facts of another without citing the authority. Every member of this association who has due appreciation of the time and labor and special knowledge required to produce a good and true illustration of the transformations and chief characteristics of an insect will appreciate this criticism. However pardonable in fugitive newspaper articles in respect of cuts which, from repeated use, have become common, or which have no individuality, the habit inevitably gives a certain spurious character to more serious and official publications ; for assumption of originality, whether intended or not, goes with uncredited matter, whether of text or figure. Nor is mere acknowledgment of loan or purchase, to the publisher, institution, or individual who may own the block or stone, what I refer to, but that acknowledgment to the author of the figure, or to the work in which it first appears, which is part of conscientious writing, and often a valuable index as to the reliability of the figure.

It were supererogation to point out to a body of this kind the value of the most careful and thorough work in connection with life histories and habits, often involving, as it does, much microscopic study of structure. The officers of our institutions who control the funds, and more or less fully our conduct, are apt to be somewhat impatient and inappreciative of the time given to anatomic work ; and where it is given for the purpose of describing species and of synopsizing or monographing higher groups, without reference to agriculture, I am firmly of the belief that it diverts one from economic work ; but where pursued for a definite economic purpose it cannot be too careful or too thorough, and I know of no instances better calculated to appeal to and modify the views of those inclined to belittle such structural study than *Phylloxera* and *Icerya*. On the careful comparison of the European and American specimens of *Phylloxera vastatrix*, involving the most minute structures and details, depended originally those important economic questions which have resulted in legislation by many different nations, and the regeneration of the affected vineyards of Europe, of our own Pacific coast, and of other parts of the world, by the use of American resistant stocks. In the case of *Icerya purchasi* the possibilities of success in checking it by its natural enemies hung at one time upon a question of specific difference between it and the *Icerya sacchari* of Signoret, — a question of minute structure,

which the descriptions left unsettled, and which could only be settled by the most careful structural study and the comparison of the types, involving a trip to Europe.

Conclusion.

I have thus touched, gentlemen, upon a few of the many subjects that crowd upon the mind for consideration on an occasion like this, — a few gleanings from a field which is passing rich in promise and possibility. It is a field that some of us have cultivated for many years, and yet have only scratched the surface ; and, if I have ventured to suggest or admonish, it is with the feeling that my own labors in this field are ere long about to end, and that I may not have another occasion. At no time in the history of the world has there, I trow, been gathered together such a body of devoted and capable workers in applied entomology. It marks an era in our calling, and, looking back at the progress of the past fifteen years, we may well ponder the possibilities of the next fifteen. They will be fruitful of grand results in proportion as we persistently and combinedly pursue the yet unsolved problems, and are not tempted to the immediate presentation of separate facts, which are so innumerable and so easily observed that their very wealth becomes an element of weakness. Epoch-making discoveries result only from this power of following up unswervingly any given problem or any fixed ideal. The kerosene emulsion ; the cyclone nozzle ; the history of *Phylloxera vastatrix*, of *Phorodon humuli*, of *Vedalia cardinalis*, — are illustrations in point : and, while we may not expect frequent results as striking or of as wide application as these, there is no end of important problems yet to be solved, and from the solution of which we may look for similar beneficial results. Applied entomology is often considered a sordid pursuit ; but it only becomes so when the object is sordid. When pursued with unselfish enthusiasm born of the love of investigation and the delight in benefiting our fellow-men, it is inspiring ; and there are few pursuits more deservedly so, considering the vast losses to our farmers from insect injury and the pressing need that the distressed husbandman has for every aid that can be given him. Our work is elevating in its sympathies for the struggles and sufferings of others. Our standard should be high, — the pursuit of knowledge for the advancement of agriculture. No official entomologist should lower it by sordid aims.

During the recent political campaign the farmer must have been sorely puzzled to know whether his interests needed protection or not. On the abstract question of tariff protection to his products, we, as entomologists, may no more agree than do the politicians, or than does the farmer himself ; but ours is a case of protection from injurious insects, and upon that there can nowhere be division of opinion. It is our duty to see that he gets it with as little tax for the means as possible. Gentlemen, I thank you.

NOTES AND NEWS.

A SERIES of experiments upon the synthetical production of cyanogen compounds by the mutual action of charcoal, gaseous nitrogen, and alkaline oxides or carbonates, at high temperatures and under great pressure, are described by Professor Hempel in the *Berichte*, and quoted im *Nature* of Dec. 18. Bunsen and Playfair long ago showed, that, when charcoal and potassium carbonate are heated to redness in an atmosphere of nitrogen, a certain quantity of cyanide of potassium is formed. Since that time Margueritte and Sourdeval have further shown that barium carbonate may be used in place of the potash, and that the barium cyanide produced may be again decomposed by steam into ammonia and barium carbonate. These re-actions afforded a theoretically continuous process for the conversion of atmospheric nitrogen into ammonia,— a process which, if it could only be worked on the large scale, would doubtless be of immense value. Unfortunately, however, only small proportions of the substances appear to enter into the re-action at ordinary pressures : hence the yield is not sufficiently large to render the process economical. Professor Hempel, however, by means of a simple pressure apparatus, has shown that the re-action is very much more complete, and, when potash is used, very energetic, under the pressure of sixty atmospheres. His apparatus consists of a strong cylinder closed at one end, and worked out of a single block of steel. The steel top screws tightly down, so as to form a closed chamber, and is pierced with two apertures,— one for connection with the compressing-pumps, and a second to admit the passage of an insulated copper rod. Within the steel cylinder is placed a smaller cylinder of porcelain, in which the mixture of the alkaline oxide or carbonate and charcoal is placed. Through the centre of this mixture passes a rod of charcoal, which is connected above with the copper rod, and below with the steel cylinder itself, in such a manner, that, when the wires from a strong battery or dynamo are connected with the projecting end of the copper rod and the exterior of the steel cylinder respectively, the rod of charcoal becomes heated to redness. The pumps are then caused to force in nitrogen gas until the desired pressure is registered on the gauge. Experimenting in this manner, it was found that the amount of barium cyanide formed in fifteen minutes under a pressure of sixty atmospheres was nearly four times that formed at ordinary atmospheric pressure, while in case of potassium carbonate the re-action was so energetic that in a few seconds the heated carbon rod itself was dissolved : hence it is evident that the formation of cyanides by heating together alkaline carbonates and charcoal in an atmosphere of nitrogen is greatly accelerated by largely increasing the pressure under which the re-action occurs.

— A well-attended meeting for the inauguration of an American Morphological Society was held in the Massachusetts Institute of Technology, Boston, on Dec. 29 and 30, 1890. Officers for the meeting were elected as follows: president, Professor E. B. Wilson; secretary and treasurer, Dr. I. Playfair McMurrich; executive committee, Professor E. L. Mark, Professor C. S. Minot, and Dr. E. A. Andrews. After the details of the organization had been completed, the following papers were read and discussed: "On the Development of the Scyphomedusæ," by I. Playfair McMurrich; "On the Intercalation of Vertebræ," by G. Baur; "The Heliotropism of Hydra, a Study in Natural Selection," by E. B. Wilson; "The Early Stages of the Development of the Lobster," by H. C. Bumpus; "Some Characteristics of the Primitive Vertebrate Brain," by H. F. Osborn; "The Development of Nereis and the Mesoblast Question," by E. B. Wilson; "The Præ-oral Organ of Xiphidium," by W. M. Wheeler; "A Review of the Cretaceous Mammalia," by H. F. Osborn; "Spermatophores as a Means of Indirect Impregnation," by C. O. Whitman; "The Phylogeny of the Actinozoa," by I. Playfair McMurrich. The following are the officers of the society for the ensuing year: president, Professor C. O. Whitman; vice-president, Professor E. L. Mark; secretary and treasurer, Dr. I. Playfair McMurrich; executive committee, the officers of the society, Professor E. B. Wilson, and Professor H. F. Osborn.

— "Iron Smelting by Modern Methods" will be the subject of the February article in the American Industries Series now running in *The Popular Science Monthly*. Every man who wishes to understand the progress of the great industries that have made the wealth and prosperity of the United States should read this series. Col. Garrick Mallery will contribute an article on "Greeting by Gesture," in which he describes many curious salutations, such as stroking one another's heads and bodies, rubbing noses, kissing, etc., practised in all parts of the world. The February number will also contain the conclusion of Dr. Andrew D. White's paper, "From Babel to Comparative Philology," and that of Professor Huxley's discussion of the Aryan question and prehistoric man.

— In *Science* for Dec. 26, 1890, p. 361, second column, seventh line from the bottom, "3,810" should read "5,810."

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Communications will be welcomed from any quarter. Abstracts of scientific papers are solicited, and twenty copies of the issue containing such will be mailed the author on request in advance. Rejected manuscripts will be returned to the authors only when the requisite amount of postage accompanies the manuscript. Whatever is intended for insertion must be authenticated by the name and address of the writer; not necessarily for publication, but as a guaranty of good faith. We do not hold ourselves responsible for any view or opinions expressed in the communications of our correspondents.

Attention is called to the "Wants" column. All are invited to use it in soliciting information or seeking new positions. The name and address of applicants should be given in full, so that answers will go direct to them. The "Exchange" column is likewise open.

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CONTENTS:

THE MINERAL WATERS OF THE YELLOWSTONE NATIONAL PARK.

THE recent publication of Bulletin No. 47, of the United States Geological Survey, containing analyses of hot-spring, geyser, and river waters from the Yellowstone National Park, is not without interest to the medical profession and to the public at large.

The waters, collected by experts employed by the Geological Survey, have been most carefully examined by Dr. F. A. Gooch, now professor of chemistry at Yale College, and Mr. J. Edward Whitfield of the survey laboratory, and represent the latest and best methods of water-analysis.

The analyses of these waters are of particular interest, because the great variety of mineral springs found in the Yellowstone, attracting the attention of all visitors to that region, suggests their use as remedial agents in the cure of disease.

Aside from the well-known resorts of the Virginias, there are but few places in the United States where natural hot waters are thus utilized. The hot-springs of Arkansas have long been known, and many cures effected by their use, combined with the care of the attendant physicians. More recently the Spas of Las Vegas, N. Mex., have been brought before the notice of the medical profession and the public generally.

Without detracting from the merits of these justly noted sanitaria, it may be stated that at neither place do the waters present as important a combination of salts in solution as those of the Yellowstone Park. Indeed, with the exception of the hot-springs in New Zealand, no waters readily accessible are known presenting the variety and remedial constituents of the Yellowstone springs. In New Zealand the government, appreciating the munificent endowment which nature has given the country in its hot-springs, has set apart certain tracts as sanitary resorts; and at the most famous resort, Rotorua, bath-houses and bathing-pools, with the usual accessories of reading-rooms and hotels, have been built at government expense, and are under the supervision of a government physician.

From a therapeutic standpoint, the analyses of hot-spring waters from the Yellowstone may be grouped as calcareous, alkaline-silicious, acid, and sulphurous.

The former, comprising the hot water of the Mammoth Hot Spring, are highly charged with carbonate of lime, which they deposit, on exposure, in the form of travertine. They resemble in composition the waters of Carlsbad, as will be seen by a comparison of the analyses of the two waters.

For bathing purposes they are less agreeable, and probably less beneficial, than the alkaline waters of the geyser basins of the Yellowstone Park.

These latter waters are generally highly charged with alkaline salts, — sodium chloride and sodium carbonate, together with silica, being the chief constituents, — but there is generally present also a small amount of sodium borate, also sodium arseniate, the latter a most valuable therapeutic agent in a variety of diseases.

The luxury of bathing in these waters must be indulged in to be appreciated. The extreme softness of the water, and the delightful freshness which one notices after the bath, render the use of the water a great pleasure. In New Zealand, where a water almost identical in composition, save that it lacks the arsenic, has been used for several years, this type of water has been found most beneficial in the treatment of gout, rheumatic troubles, and sciatica. In France the curative properties of waters carrying arsenic in solution are fully recognized, especially for the cure of certain forms of nervous and skin diseases. While the Yellowstone waters contain a little less arsenic than those of the French springs at La Bourboule, there is no reason to doubt their usefulness for similar diseases. At present the only water of this class utilized for bathing purposes is that of the Hygeia Spring, supplying the baths of the hotel at the Firehole, or Lower Geyser Basin.

This water carries three-tenths of a grain of sodium arsenic to the gallon. It has been tried by the writer, and found a most delightful water for bathing, but no invalids have yet tested its virtues. Springs of this character are, however, very numerous, and their waters might be easily utilized for bathing.

The acid waters, carrying free hydrochloric acid, are less numerous in the park, but many springs of this character are found at the Norris Geyser Basin. The waters may be perfectly clear, as is the case with the outflow of the Echenis Geyser and the discharge from Green Spring, or turbid, and charged with more or less sulphur, as is more frequently the case. Such waters have achieved a considerable reputation in New Zealand as a tonic and alterative, particularly in diseases of the liver and in functional troubles of females.

They also exert a powerful effect upon the body in all skin-diseases, but are probably less useful than the sulphurous waters in such cases. At present no waters of this character are utilized for baths, but could be readily led into suitable bath-houses at the Norris Basin. This locality is indeed the best suited for a sanitarium of any of the geyser basins of the park, as all the varieties of waters occur here, save the calcareous.

Sulphurous waters are very familiar, though those of the Yellowstone are particularly strong. The Mammoth Hot Spring waters, though smelling strongly of sulphur at the vent, possess little, if any, of that important constituent when led into baths, for it is all deposited about the vents and upon the algæ growing in the waters; but excellent examples of this type are found at the Norris Basin, as well as elsewhere in the park.

Now that the roads and hotel accommodations in the park are so good, and the region so easily reached in Pullman coaches and with dining-cars, it is to be hoped that the waters of these springs may bring relief to many sufferers.

WALTER HARVEY WEED.

LETTERS TO THE EDITOR.

**** *Correspondents are requested to be as brief as possible. The writer's name is in all cases required as proof of good faith.*

The editor will be glad to publish any queries consonant with the character of the journal.

On request, twenty copies of the number containing his communication will be furnished free to any correspondent.

Time-Measuring among Savage Peoples.

THE question has arisen in the National Museum whether the American aborigines or any other savage peoples have any mechanical devices for measuring the time of day or portions of the day. I do not now allude to calendars, of which there are many, nor to observation of dawn, sunrise, a little after sunrise, near noon, noon, etc., based on the diurnal movement of the heavenly bodies, but to primitive dials and the like. I have heard of the Montaguai's practice of setting a staff in the snow and marking the shadow, and of the Pueblo habit of marking the path of a sun-ray across the floor, but my information is not first-rate. My familiarity with the African and Insular peoples is limited; but it is designed to set up in the National Museum an elaborative series to illustrate time-keeping, and we are anxious to know what manner of invention should stand at the beginning of the series.

OTIS T. MASON.

Washington, Jan. 10.

Professor Ferrel and American Meteorologists.

IT would seem to be high time that some one having authority should read the riot act to a number of American meteorologists. The views lately advanced by Dr. Hann, that cyclones (excepting those of tropical regions) have their origin rather in the great general movements of the upper atmosphere than in the ascensional movement of relatively warm and moist air and the consequent vapor condensation, may or may not stand the test of a more extensive and critical series of temperature studies than those made in 1889, but it is none the less incumbent upon American meteorologists to treat with proper courtesy the conscientious and life-long labors of a fellow-countryman; and it is but scant courtesy to exhibit to the world an eagerness to drag into prominence and accept seriously a new theory of cyclonic genesis, when such a theory lacks in every way extensive and careful study, and is really but little more than a mere possibility suggested by an eminent foreign meteorologist, when he found in certain temperature observations a somewhat marked difference from those which the accepted theory seemed to him to require.

There may be "thermic," and there may be dynamic, cyclones; but the observations should be numerous and trustworthy before it is claimed that such a distinction exists, and before we seriously accept the very radical view that temperatures in cyclones are determined by the motions of the air. A thorough series of temperature determinations at different parts of the storm, as a mechanism, is needed, and should be offered. Especially is this demanded when the acceptance of the new view implies a partial remodelling, at least, of a theory that is of long standing, and has the sanction of one of the best equipped minds of the many that have tackled meteorological problems. Should occasion require, Professor Ferrel can doubtless successfully defend the views he holds; but, for the benefit of some who may not be aware of his methods of work, it may be not out of place to say here that nothing from his hand is the result of haste, but, on the contrary, the result of mature thought, and patient, careful, deliberate study of the best scientific information at his command.

With all possible deference to Dr. Hann's eminence in matters meteorological, it is to be questioned whether a series of temperature observations at some fourteen stations, seven of which have an altitude of over two thousand metres, for only two storms (the barometric maximum of Nov. 12–24, and the minimum of Oct. 1), prove any thing, after all, but that it is quite possible to find temperatures higher than the normal when lower ones might be expected. But this abnormality is but a slim support for a new theory, nor does it disprove the old. The air in the high area late in November was apparently warmer than the air in the "low" at the beginning of October; but that does not prove that the mean temperature of the air in any and every maxima is always higher than the mean temperature of any and every extra-tropical minima (it is conceded that the new theory will not hold for tropical storms). Dr. Hann claims that seven of these alpine stations have an elevation over two kilometres above sea-level. Yet it may be an open question if these heights give the conditions which he sought, more particularly if we remember that certain of the cirri clouds certainly have an elevation of not less than eighty kilometres, and a two-kilometre temperature observation may give but an uncertain indication. We can even find at surface stations abnormalities, that, if misinterpreted, might lead us to doubt a great many of our accepted views in the matter of atmospheric temperature. Mr. Kingston,[1] director of the Toronto Observatory in 1868, called attention to the fact that the twelve-year normals (1841–52) were not applicable to observations of later years, and, according to five year normals, it was easy to show that January was warmer than February, etc.; and Schott shows in a table how, from 1841 to 1850, February was colder than January at New Haven, Toronto, Philadelphia, Charleston, and Savannah, while from 1851 to 1860 the reverse holds true.

It is therefore, it seems to me, only fair to insist that American meteorologists demand full and most thorough evidence before seriously considering the question of modifying present theories; more particularly, too, when an unintentional but none the less real disposition exists in certain quarters to speak carelessly of Professor Ferrel and his work, and to deny him his proper place.

Not a bad example of this carelessness appears in a translation by E. F. Bamber, in the *Philosophical Magazine* for December, 1890, of Werner von Siemens's views on a general system of winds of the earth. The eminent physicist, in refuting the statement of Dr. Sprung in a recent paper in the *Meteorologische Zeitschrift*, that he attempted, like Ferrel, to found on theoretical calculations a theory of the general system of winds on the earth, disclaims in all modesty a sufficient proficiency in the higher mathematics to do this, but then immediately adds, it appears to us somewhat illogically, that he "considers this method altogether inappropriate." He therefore repudiates the charge that "he sought, like Ferrel, to demonstrate by means of calculation an original state of atmospheric motion in order to afterwards base his further speculations thereon."[2] There is no intentional intimation here, we take it, that Ferrel's views are based on a supposition more or less hasty and uncertain, and there is therefore little occasion for the rejoinder that any such intimation indicates a lack of familiarity with Ferrel's work; but it ought to be felt and recognized, especially by American meteorologists, that experimental fact rests at the bottom of every natural law

[1] See Schott's Tables, p. 199.

[2] Sitzungsberichte d. K. Preuss. Akad. d. Wiss. zu Berlin, 1890.

discussed by Ferrel, that in every case the latest and most accurately determined physical constants are used, and that the theoretical deductions, while simply offered as such to be tested, are strictly the results of mathematical analyses. If in time these appear inadequate, the measure of praise for the man and his work may be diminished, but only in proportion as it is remembered that meteorological data and laws were in a condition more or less chaotic when he took up his labor of developing these into a consistent harmonious science.

ALEXANDER MCADIE.

Washington, D.C., Jan. 9.

Cyclones and Areas of High Pressure.

I HAD supposed that Professor Davis would give some explanation of the argument against the condensation theory of cyclones deduced from the comparisons of the temperatures in cyclones with those in high-pressure areas. He commences with a citation from my book, in which I state that the high pressures in the north-west sides of cyclones in the higher latitudes in winter are caused mostly by their lower temperatures, and consequently greater densities. He thinks the high pressure over the Alps in November, 1889, is a typical case of all such high-pressure areas. While I do not so regard it, yet, for the sake of brevity, I will here concede it, and consider merely this supposed typical case. Over the Alps, during the last five of the fourteen days of the existence of this high pressure, the temperature on the summits of the Alps was found to be several degrees warmer than the normal temperature of the season. There are no observations to show how high this abnormal temperature extended, but I am willing to admit that it may have extended up to a considerable altitude. Professor Davis, because this temperature is found to be above the normal a few degrees, maintains that the descent of the air is not due to its being heavier than the surrounding air, thus assuming that the surrounding temperatures at a distance at the time are the same as the normal temperature, notwithstanding the well-known great and long-continued departures from the normals which frequently occur over large areas of the country. But it is not necessary that this body of heated air in high-pressure areas should have a temperature lower than the surrounding temperatures even; for if the great vertical extent of air above it has a temperature only one or two degrees lower than the surrounding temperatures on the same levels, which gives rise to a descending current, the air below, if it even has a little higher temperature than the surroundings, cannot rise up through the descending current, but must be forced downward. But suppose it were clearly established that the air in a high-pressure area extending hundreds of miles had a lower temperature than the surroundings even, and not merely the normal of the season: how is the greater pressure and the descent of the air to be accounted for? Professor Davis has never hinted at a probable explanation merely. The deduction, therefore, from a few surface observations merely in a very limited region, that the air over a large area, and extending to the top of the atmosphere, is warmer than the surrounding air at a great distance in all directions, especially where these few observations are found to give a temperature above the normal merely, and not above the surrounding temperatures at the same levels, should be received with great caution; for, if there were even a well-established theory to account for the descent of the air under these circumstances, these observations could scarcely be regarded as having any weight in confirmation of such a theory.

In what precedes I have gone upon the assumption that a lower temperature is the only cause of the descent of the air in high-pressure areas. While I regard this as adequate to account for it, I have never said or thought that it is the only cause, but simply the principal cause. I think there are other causes, especially in the origin of these high-pressure areas, which, for our present purpose, it is not necessary to discuss here.

Professor Davis says, "Records of temperature made on high mountain-peaks furnish the best means of testing the convectional theory of cyclones, for, even if all other tests were successfully borne, failure under this test would be fatal to the theory." By "convectional theory of cyclones" I understand him to mean the condensation theory, which requires the air in the ascending current to be warmer and lighter than that of the surroundings at the same levels. Now, this theory can neither be established nor overthrown by any such tests. Cyclones are usually several hundred, sometimes a thousand and more, miles in diameter; and to prove that the air over so large an area up to the top of the atmosphere, or at least up to high altitudes, has a higher or a lower temperature than its surroundings, would require numerous stations of observation at many different levels, not only over this large area, but also all around this area at great distances. The condensation theory requires that the temperature of the air in a cyclone must be greater, in a general way, than that of the surrounding air; but this does not mean that there are no places within the cyclone, especially on the earth's surface, with lower temperatures than those of many places outside. In the theoretical treatment of a cyclone we have necessarily to assume certain regular conditions of uniform temperature at the same distances in all directions; but I have always been careful to explain that such conditions are never found in nature, but generally only rough approximations. In a large cyclone there is a great difference between the north and south sides, due to difference of latitude, which is taken into account in the general motions of the atmosphere, and so must be excluded in the treatment of the cyclones, and the differences of temperature only with reference to corresponding temperatures outside of the cyclone on the same latitudes must be considered. Besides, the temperatures vary all around the cyclone, not only on account of difference of latitude, but likewise from various abnormal causes. It must be expected, therefore, in comparing inside temperatures with the surrounding ones, especially surface temperatures, that there would be numerous cases in which those within would be found lower than many of those in the surroundings. The theory only requires that there shall be a predominance of higher temperatures in the interior. Besides, the conditions of a cyclone need not extend down to the surface at all, and, in fact, mere surface conditions generally have little or nothing to do with a cyclone. If the necessary conditions exist at altitudes only considerably above the earth's surface, the air is thrown into a great whirl or gyration, which relieves the air below of a part of the pressure upon it, and increases the pressure round about; so that this air tends to rise up, just as the water does in a suction-pump, and the surrounding air flows in to take its place; and in flowing in it assumes a gyratory motion, not only from the deflecting force of the earth's rotation, but likewise from the action of the air above by means of friction, so that it is brought into the general vertical and gyratory circulation. But suppose that it could be shown that the air in a cyclone is mostly or entirely of a lower temperature than the surrounding air at all altitudes, and yet ascends, as it always does: how is this strange phenomenon to be accounted for when there is no force, either real or imaginary, to cause it to ascend?

Professor Davis thinks that the snow-fall on the Alps at the time of the cyclone of Oct. 1, 1889, had little effect in lowering the temperature, on account of the wind; but this is one of the causes which Dr. Hann gave, a few years ago, of the lower surface temperatures in cyclones. The air, in being forced up the mountains on the windward side, is expanded and cooled below the temperature of the air generally on the same level. Another reason which he assigned was, that as the lowest pressure above lags behind that below, as was shown by Loomis, and first explained, I think, by Dr. Hann, the cold north-westerly winds set in above rather before the lowest pressure-point is passed. The real centre of the cyclone above is not that of lowest pressure.

I admit that it is not strictly logical to assume that two theories, or two kinds of forces, may not be such as to give the same effects, especially where nothing is known of the nature or manner of application of the one kind; but still this is extremely improbable. As the general motions of the atmosphere, cyclones, and tornadoes, are all very much alike, consisting of gyrations around a centre, — and it is admitted that in the first and last the air rises where it is warmest and lightest and because this is so, and that this is even the case with cyclones in the lower latitudes,— we should hesitate in making an exception in the case

of cyclones in the higher latitudes, because a few surface observations merely of temperature, which, as has been shown I think, should have no weight, seem to indicate that the complete conditions of a cyclone, upon the condensation theory, do not exist.

Mr. Clayton, in his communication, sets out in a very commendable way by discarding mere authority in scientific questions. He, however, proceeds to give two columns of citations from different authorities. But the most of this is entirely proper; for we have to depend more or less upon authority for observational data, and it is only where the decision of a question depends merely upon the use and application of scientific principles that mere authority should be discarded. All observations, however, should be well considered and weighed, especially where they seem to conflict with well-established scientific principles. I have been familiar with all of Loomis's meteorological papers, and I do not call to mind any cases in which his results deduced directly from observation seemed to be in conflict with any theories which I have advocated, but of course there are some things which I cannot satisfactorily explain. I have always made numerous quotations from Loomis's papers in confirmation of my theories It is a little singular, however, that Mr. Clayton should cite some of the same things against me. From some of Loomis's theoretical deductions from the observations I dissent.

With regard to the comparisons of observations at Denver and Pike's Peak, both merely surface observations at a long distance apart, in order to show whether the air is in a state of stable or unstable equilibrium over an area hundreds of miles in diameter, it is not necessary for me to add any thing more to what I have already stated on that subject. These cases were mostly in the summer season, when mountain-peaks are cooler than the surrounding air at a distance, and when lowland stations are abnormally heated. and the vertical temperature gradient, for some distance from the surface, large. If the lower temperatures had been taken a little above the surface, and compared with one vertically above it, no unstable state, probably, would have been indicated when, as is stated, no extraordinary disturbances occurred The reason why most of these cases of unstable, and approximately unstable, states occurred in May, I have explained in my book. Whether heated dry air has much ascensional force depends upon the state of the air. In the stable state it can only ascend until it becomes cooled down to the temperature of the surrounding air at a distance on the same levels. In the unstable state, the higher it ascends, the warmer it becomes relatively to the surrounding air; and so, of course, it rushes up with great violence until the stable state is again restored.

The fact which Loomis has established, and which is a matter of common observation, that very heavy rains do not continue very long, is very reasonable; for the more rapidly the store of energy in the uncondensed vapor is spent, the sooner, of course, must the store of energy become exhausted.

I have been at great pains to show that the unstable state, which gives rise to cyclones and tornadoes, may be induced in perfectly dry air; and I have cited Loomis in conformation of this, when he shows that cyclones of moderate barometric depression in the centre, and without any violence, do exist. But Mr. Clayton brings in the same thing against the condensation theory, under the impression, I suppose, that, because I call the theory of cyclones the condensation theory in deference to Espy, I consider vapor and its condensation entirely indispensable. The vapor is a very essential part, and without it cyclones would, no doubt, be of much less frequent occurrence, and would have little violence. Loomis has shown that when there are cyclones in dry weather, with little or no rain, the depressions are small. These take place mostly in the summer season, when the air over a large area becomes much heated; and although the ascent of air over this region is not sufficient to give rise to much rain, or even cloudiness perhaps. yet it is sufficient to cause haziness in the atmosphere, in which state the heat energy is absorbed directly from the sun's rays, instead of getting it indirectly from condensation after it has been absorbed in evaporation. Mr. Clayton cites a number of authorities to show that there is a body of warm air, a little above the earth's surface, in areas of high pressure, and that the vertical temperature gradient here is small, much less often than in cyclones. I have never denied this. It is simply storming a camp in which I am not to be found. More than six years ago, in "Recent Advances in Meteorology," I gave seven cases of this sort, one in which detailed observations were given to show that the vertical temperature gradient may become inverted. The same is given in my recent work.

Mr. Clayton thinks that Dr. Hann's recent investigations of cyclones in the Alps should add a link to the chain of evidence that the temperature of the air-column as a whole is lower in cyclones than in the surrounding air; but, if this is even admitted, where are the other links? So far as I can see, they all seem to be "missing links." He also gives his views with regard to various other things, which is well enough if they are not intended as arguments, and they do not seem to be. But still it is of much more importance to know what he can prove and establish than to know what he thinks. He thinks that mechanical action has much to do with the origin of storms; but what this means, I am unable to say. The mere origin of a cyclone, although of importance, is of little importance in comparison with the great question of where the energy comes from to support the cyclone after it has been originated.

Finally, Mr. Clayton proposes three questions for my answer. To the first and second I answer emphatically, "No." If Mr. Clayton thinks that a cyclone can originate and be maintained in this way, let him show in what way. But let him remember that he is not to commence with his high areas and his troughs, for this is not a normal condition of the atmosphere, but let him first account for these. and then proceed to show how the air in flowing into his trough is thrown into a gyration; and as the air in this area of gyration, according to the new theory, is heavier than the surrounding air, and at the same time rises up, let him especially show where the energy comes from to support the gyration and force up the heavier air in the interior. I do not say that in such a case there would not be a certain very small amount of gyratory movement produced by the flowing of the air into the trough while it was being filled up, as it would be at once if there were no restraining force to keep the air from the high pressures on each side from rushing in. But such high-pressure areas continue often a long time, and do not fill up the troughs; and the question is, what maintains them? I have fully explained all this at various times upon my principles, and I now leave it to him to explain upon his. I commence with a normal state of air without high-pressure areas and troughs of low pressure, and show how the unstable state is induced, how from this the cyclone originates, and how the gyrations cause a wave of high pressure all around, and, where there are two cyclones, how the ridge of high pressure between is caused. The low-pressure between two cyclones, together with other irregularities of pressure, permanent or otherwise, in some rare cases, gives a very oblong low-pressure area, or trough. Mr. Clayton proceeds in the reverse order, and commences with the high pressures without first accounting for them, which he makes a basis of his whole process. The world is supported upon the shoulders of Atlas, and Atlas upon the back of a tortoise; but the question still arises, upon what does the tortoise stand? Let Mr. Clayton first show upon what his tortoise stands.

With regard to Mr. Clayton's last question, I know nothing with regard to the circumstances of the cyclone to which he refers. It was in the winter, when surface temperatures are very low, and vertical temperature gradients small, and even reversed sometimes near the earth's surface. This, however, does not affect the gradient, estimated from a little distance above the earth; but I have said so much with regard to the inadequacy of a few surface observations at the bottom of the great ocean of atmosphere to prove that the air, or no part of it above, is not warmer than the surrounding air, all of which is just as pertinent in this case, that certainly nothing more can be required. As I have said before, the mere surface condition may have little or nothing to do with a cyclone. But suppose I cannot explain it, as Mr. Clayton seems to think, "upon the assumption of a higher mean temperature of the air-column within the field of the cyclone:" how does he explain it upon the assumption of a lower mean temperature and heavier air-column? He proposes his question with an

air which would indicate that he had completely explained the phenomenon upon his theory, whereas there has never been even an attempt made to explain any thing by it.

The law of gravitation, suggested by the fall of an apple, was withheld by Newton for a number of years, because, on account of incorrect data, it was not confirmed by observation. With the reserve and caution characteristic of a true philosopher, he thought it should be fully tried and tested first. But now we have a theory thrust upon us for our assent which has not been developed, and applied in the explanation of a single phenomenon in the local disturbances of the atmosphere; and yet I am censured for thinking that there has been entirely too much haste in the matter, and that it should first have been shown that it will at least account for a few of the observed atmospheric phenomena. Let the advocates of this theory, if it can be so called, take up the matter now, and show that it accounts for the phenomena as well as, or better than, the condensation theory. Let them give me a chance to look into the workings of this new theory.

WM. FERREL.

Martinsburg, W. Va., Jan. 10.

BOOK-REVIEWS.

Tycho Brahe: a Picture of Scientific Life and Work in the Sixteenth Century. By J. L. E. DREYER. Edinburgh, Adam & Charles Black. 8°. (New York, Macmillan, $3.50.)

THIS is a work of much value to students of the history of science. Tycho Brahe holds a prominent place in the annals of astronomy; and he was, moreover, a member of the Danish nobility and a man of considerable means, with a wide circle of acquaintances and many opportunities for travel. Hence his life was more dramatic and fuller of incident than the lives of scientific men usually are; and Professor Dreyer has here related it in an interesting way. The book is well written, with great care in collecting and sifting the facts, and with an evident desire to be just to all parties. The early life and studies of Tycho are described somewhat briefly; but a full account is given of his early attempts at astronomical observation and of the endowments given him by King Frederick II. to enable him to pursue his chosen work. The Island of Hveen, which was assigned him to hold during the king's pleasure, became the scene of his most important discoveries; and the income it afforded, together with certain other revenues placed at his disposal by his royal friend and patron, enabled him to hire assistants and to prosecute his work vigorously for many years. But after the death of Frederick the authorities were less favorable to Tycho; so that at last his endowments were taken from him, and he left Denmark for a new field of labor under the German emperor at Prague. Professor Dreyer gives a very good description of the Island of Hveen, and the facilities available there for astronomical work, and then endeavors to explain how and why Tycho Brahe lost his position there, — a misfortune due quite as much to Tycho's own faults as to the disfavor of the authorities. His new station at Prague is also well described; and one of the most interesting passages in the book is that relating the meeting of the veteran Tycho with the young Kepler, an event of such significance in the development of science. Indeed, this meeting was the most important result of Tycho's residence at Prague, which was soon terminated by his death in his fifty-fifth year.

Of Tycho Brahe's scientific achievements, Professor Dreyer gives a full and detailed account. He was an observer rather than a thinker, and his biographer thinks that his observations could hardly have been surpassed in accuracy but for the invention of the telescope. The instruments he employed, many of which were devised by him, are described with some minuteness, and the importance of his observations as a basis for the theories of Kepler and Newton is clearly shown. Tycho's most important labors, in Professor Dreyer's opinion, were those relating to the

movements of the moon and the planets, his catalogue of a thousand stars, and his observations of comets, which were the means of refuting Aristotle's opinion that these bodies belonged to our atmosphere. Considerable space is devoted to Tycho's work in astrology, to which he gave much attention, and in which his faith, though not as enthusiastic as that of some men, was never abandoned. Altogether, Professor Dreyer's work is worthy of its theme, and will hold an honorable place among biographies of scientific men.

Introduction to the Study of Federal Government. By ALBERT BUSHNELL HART. (Harvard Historical Monographs, No. 2). Boston, Ginn. 8°. $1 net.

WE noticed the first of these monographs a short time since, and we are now glad to receive the second. It is only an introductory work, forming a pamphlet of two hundred pages, and the author tells us in his preface that it is to be followed in due time by an extended treatise on the same subject; yet it is of real value in itself. Professor Hart opens his work with a discussion of the nature of federation and of the various types of federal government that are known in history, — a discussion that shows a clear view of the questions involved, and considerable power of philosophic thought. He next proceeds to a brief but very clear account of the ancient and mediæval confederations from the first conception of the federal idea among the Greeks to the Holy Roman Empire, then gives a description of the four great existing federations, — those of the United States, Switzerland, Germany, and Canada, — and closes with a short chapter on the Latin-American federations, in which he has no great confidence The monograph is written in a good style, and shows throughout not only a careful study of the facts, but also the fruits of thought and meditation, which are not always found in American historical writings. Besides the text of the work, there is a long and elaborate appendix, containing a conspectus of the four chief existing federations mentioned above, arranged in parallel form, and giving the provisions of each of the four constitutions on every important point. This appendix thus presents a large amount of information in a form convenient for reference; and there is also another appendix containing a bibliography of federal government. Altogether, the pamphlet is a creditable one; and historical writers in our other universities will have to do better than they have done heretofore if their work is to rank on a level with these Harvard monographs.

AMONG THE PUBLISHERS.

THE latest number of the "Proceedings of the United States Naval Institute" opens with an interesting article on the protection of the hulls of vessels by lacquer, detailing the results of experiments on several Japanese men-of-war. The experiments seem to prove that lacquer is a perfect protection against the action of sea-water so long as the coat remains unbroken.

— Norman W. Henley & Co., publishers and importers of scientific and technical books (150 Nassau Street, this city), announce for immediate publication "Rubber Hand-Stamp Making and the Manipulation of Rubber," by T. O'Conor Sloane, A.M; and "Arithmetic of Electricity," by the same author. They have also in preparation the "Manufacturers', Mechanics', and Business Men's Assistant," by Benjamin Franklin, LL.B.

— Among the principal articles in the *Journal of the Military Service Institution* for January are the following: "A Practical Scheme for Training the Regular Army in Field Duties for War" (a prize essay), by Lieut. Read; "A Proposed Change in Artillery School Methods," by Lieut. Hunter; "Modern Bobadilism," by Capt. Chester; "Strategy, Tactics, and Policy" (a summary), by Lieut. Bush; "The Gyroscope and 'Drift,'" by Lieut. Richmond; "Practical Education of the Soldier," by Lieut. Parkhurst; and "The Battle of Plattsburg," by Gen. Macomb.

Publications received at Editor's Office, Jan. 5-10.

OPTICIAN, The. Vol. I. No. 1. *m.* New York, Frederick Boger. 16 p. 8°. 50 cents per year.

SCUDDER, H. E. Fables and Folk Stories. Part II. (Riverside Literature Series, No. 48.) Boston and New York, Houghton, Mifflin, & Co. 200 p. 16°. 15 cents.

SZCZEPANSKI, F. v., *ed.* Bibliotheca Polytechnica. Directory of Technical Literature. New York, Internat. News Co. 80 p. 12°.

U. S. GEOLOGICAL SURVEY, Ninth Annual Report of the, to the Secretary of the Interior, 1887-88. Washington, Government. 717 p. 4°.

UPHAM, W. Report of Exploration of the Glacial Lake Agassiz in Manitoba. Montreal, W. F. Brown & Co. 156 p. 8°. 25 cents.

VOGDES, A. W. A Bibliography of Paleozoic Crustacea from 1698 to 1889. Washington, Government. 177 p. 8°.

WATKINS, J. E. Report on the Section of Transportation and Engineering in the U. S. National Museum, 1888. Washington, Government. 5 p. 8°.

WEED, W H The Formation of Travertine and Siliceous Sinter by the Vegetation of Hot Springs. Washington, Government. 62 p. f°.

WILSON, T. A Study of Prehistoric Anthropology.— Hand-Book for Beginners. Washington, Government. 75 p. 8°.

—— Results of an Inquiry as to the Existence of Man in North America during the Paleolithic Period of the Stone Age. Washington, Government. 26. 8°.

WRIGHT, G. F. The Glacial Boundary in Western Pennsylvania, Ohio, Kentucky, Indiana, and Illinois. (Bull. U. S. Geol. Surv., No. 58.) Washington, Government. 112 p. 8°.

CALENDAR OF SOCIETIES.

Anthropological Society, Washington.

Jan. 6.—Thomas Wilson, Stone Implements of Curious Forms from Breonio, Italy; W. Woodville Rockhill, Laws, Customs, and Superstitions of Korea.

Appalachian Mountain Club, Boston.

Jan. 14.—Frank O. Carpenter, The Stratford Mountains, New Hampshire; Frank W. Freeborn, Adirondack Paths.

New York Academy of Sciences.

Jan. 12.—John I. Northrop, The Results of a Recent Collecting Trip to the Bahamas (illustrated with specimens).

Jan. 19.—A. J. Du Bois, Science and Miracle.

Wants.

Any person seeking a position for which he is qualified by his scientific attainments, or any person seeking some one to fill a position of this character, be it that of a teacher of science, chemist, draughtsman, or what not, may have the 'Want' inserted under this head FREE OF COST, *if he satisfies the publisher of the suitable character of his application. Any person seeking information on any scientific question, the address of any scientific man, or who can in any way use this column for a purpose consonant with the nature of the paper, is cordially invited to do so.*

WANTED.—A situation in a scientific house by a graduate of the scientific department of the Jersey City High School as Geologist, Mineralogist or as assistant teacher in Physics or assistant to a chemist. Best of references. E. W. PERRY, 582 Third Street, Jersey City, N. J.

A UNIVERSITY GRADUATE IN SCIENCE, at present a junior student in medicine at the University of Michigan, desires a position as teacher or laboratory assistant. Special preparation in Zoology, Histology, Physiology and Human Anatomy. Will also teach, if desired, Physics, Chemistry, Botany, Entomology, Embryology, Bacteriology and Sanitary Science. References and full particulars on application. H. B., 20 East Jefferson St., Ann Arbor, Mich.

WANTED.—A young man wishes a position as instructor in the sciences in some school or college. Chemistry a specialty. Would like to hear of some position with a private or manufacturing chemist, in which his knowledge of chemistry would help him. Address E. E. TOWNE, B.A., 272 Worthington St., Springfield, Mass.

WANTED.—There being a considerable annual income for the purchase of books for the Museum Reference Library of Iowa College, it is desirable to have at hand any and all circulars, specimen sheets, catalogues, etc., of all works on Natural History in general, both foreign and domestic. Circulars of museum supplies, apparatus, etc., etc., desired also. State terms. Address ERWIN H. BARBOUR, Box 1333, Grinnell, Iowa.

WANTED.—A situation as Analytical Chemist is desired by the assistant of the late noted scientist, Dr. Cook, during his 15 years survey of the State of New Jersey. Highest testimonials furnished. Address Prof. EDWIN H. BOGARDUS, New Brunswick, P, O. Box 224, care Prof. F. C. Van Dyck.

COULD some one inform me what the ingredients and origin of asphalt as used for street-paving and gathered at Trinidad are? Also how gathered and shipped by natives, and mode of refining by the Warren-Scharf Co. of New York and the Barber Co. of Washington? G. KNIPER, 28 Gunn Block, Grand Rapids, Mich.

Exchanges.

[Free of charge to all, if of satisfactory character. Address N. D. C. Hodges, 47 Lafayette Place, New York.]

I would like to exchange first six vols. Roscoe and Schorleminer's Chemistry, and Vols. II. and III. of "Education" for American Naturalist for 1889 and 1890, or works on Zoölogy. C. Dwight Marsh, Ripon College, Ripon, Wis.

I have a good supply of Eastern Coleoptera which I wish to exchange for species not in my collection. Melandryidal and Oedemeridal especially desired. Joe C. Thompson, Rosebank P. O., Box 73, S. I.

I wish to exchange Eastern *Lepideptera* for those that I do not have, particularly those found in the South. Jos. F. Crandall, Honesdale, Wayne Co., Pa.

To exchange, 1890 Seeger and Guernsey Cyclopedia, containing a complete list of the manufactures and products of the U. S., and address of first hands. cost $6. David R. Lewis, Saybrook, Ill.

For exchange—Nice specimens of Unios alatus, trigonus, parvus, occidens, anadontoides gibbosus, rectus, verucosus, gracilis coccineus, ventricasus, multiplicatus and plicatus cornutus. Margaritana confrogosa, complanata, rugosa. Anadonta edentula, decora, corpulenta, and about 300 of the beautiful Ana. suborbiculata. Wanted: Unios from all parts of the world, and sea-shells. Address Dr. W. S. Strode, Bernadotte, Ill.